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by many other workers whether they call attention to the recapitulation shown or not.

As a whole M. Cossmann's criticism shows a total misconception of modern methods in phylogenetic study and even the illustrations which I have used to point out likeness or difference in descent are to him, judging as he does by the standards of the older conchologists, only so many offences against the good old fashioned rule of putting together species that are alike in the adult and ignoring "simples et légères modifications dans l'ornamentation de la spire." A careful comparison of the detailed figures in my paper will, however, show that the modifications are not slight in cases where genera are separated.

As might be expected, the choice of a genotype from the work of a pre-Linnean author is questioned. This choice arose from the difficulty of applying the established rules of nomenclature in such a manner as to meet the approval of all students of the subject. Bruguière, the first post-Linnæan author to use the binomial nomenclature in connection with *Cerithium*, did not select a genotype, and Lamarck chose, at different times, two of Bruguière's species as illustrations of the genus. At present one eminent authority chooses as genotype the first of Lamarck's selected species, while another chooses the second and a third suggests a choice from Bruguière's many species. If the general consensus of opinion finally fixes upon *Pseudovertagus aluco* or *Cerithium? nodulosum* instead of *C. tuberosum* as the type of *Cerithium* it would simply necessitate the choice of a new name for the group represented by *C. tuberosum*, *C. adansonii*, etc., which were the first to be described, and which have long been known by that name. However, the name applied to a natural group is of minor importance. The point of especial importance is that such shells as *Pseudovertagus aluco* and *Cerithium echinatum*, for example, can not be classed together in the same genus since their ontogeny shows that they have an entirely different ancestry. The main object of my paper is to trace the phylogeny of a natural group and to show the methods by

which relationship should be determined.

In summarizing I would emphasize the following three points:

1. A natural classification should be based on community of descent.
2. In tracing descent the whole ontogeny is a more reliable guide than a single final stage of it.
3. There is sufficient evidence in favor of the theory that ontogeny recapitulates phylogeny to make it the only safe means of determining relationship.

ELVIRA WOOD

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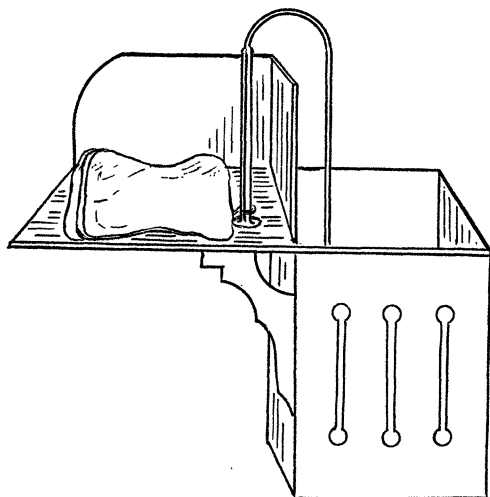
May 31, 1911

A NEW RACK FOR INDIVIDUAL TOWELS

THE "common towel" is a problem which sanitarians so far have failed to solve. This is especially true of the roller towel so often found in public places. The fact that these towels are dirty and unattractive, if not repulsive, is comparatively unimportant, for if the real facts were known it would be understood that many cases of disease are transmitted by this means. In the better places where the wash room can have frequent attention, small individual towels can be used. In a good many places, however, it seems impossible on account of the expense, due to those lost and stolen. One attempt to solve the problem has been the substitution of paper for cloth towels. In the minds of most people, however, these paper towels are not satisfactory, although, of course, they are perfectly hygienic.

An attempt has been made at the University of Wisconsin to solve this problem and towel racks have been designed which make it possible for each person to have an individual towel. This rack is very simple, as is seen in the accompanying illustration. It consists essentially of a shelf on which are placed a pile of the small, clean towels, and just below this and at one side is a receptacle or basket into which the dirty towels are placed. Connecting the shelf and the basket is a rod with a goose neck. The towels are provided with a perforation or loop through

which this rod passes, so that when a towel has been used it is slipped over the rod and



allowed to drop into the basket. This rod is ordinarily held in place by a nut, but it might be provided with a lock. It would thus be impossible to remove the towels by any one not provided with a key without tearing them.

This rack has been used at the university, in its various toilet rooms, for some months with much satisfaction.

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QUOTATIONS

THOUGHT-TRANSFERENCE

A CURIOUS offer, or challenge, has been appearing recently in our columns. An unnamed inquirer asks for "satisfactory proofs of so-called thought-transference"; and, as such proofs have not been forthcoming in response to applications to "the leading authorities and writers of repute on the subject," a reward of £1,000 is now offered to any one who will furnish them. We understand those who use the expression "thought-transference" to mean by it that, under conditions at present undetermined, the physical changes underlying the thought processes of a human brain may be brought into such relations with some unknown medium of communication as

to be conducted through its agency to another human brain, and to produce in the latter changes, and consequently thoughts, analogous to or identical with those preexisting in the former; the process being somewhat analogous to the communication of Hertzian waves from their source to a suitable receiver at a distance. The foundations of any such belief must manifestly rest upon the instances in which it is said that human beings, separated by distance, have been simultaneously the subjects of thought impressions of a similar or identical character, on matters important or interesting to both; and it is equally manifest that no "proof" of "transference" in such cases could possibly be given. The instances, or supposed instances, have never been examined with sufficient care by competent persons to exclude the innumerable possibilities of coincidence, and it is even doubtful whether any care which could be taken, after the alleged event, would be sufficient for the purpose, or could avoid the operation of "the myriad shafts of chance." The only conclusive proof would be by the intentional reproduction of the occurrence; and in order to accomplish this it would first be necessary to determine with scientific precision what were the conditions of success. If there can be a transference of the kind alleged, it must occur as a result of a state of things which, if its nature were precisely known, could be reproduced with certainty; but which, so long as it is uncertain or undefined must continue to elude observation and to baffle experiment. If "proof" be desired, it should be sought by endeavors to reproduce in a physical laboratory the circumstances which have given rise to the stories about thought-transference.—The London Times.

SCIENTIFIC BOOKS

Quantitative Mineralogical and Chemical Composition of Granites and Gneisses.
By Professor T. TCHIRWINSKY. Moscow, 1911. 8vo. Pp. vii + 659, 4 plates.

An important work by Professor T. Tchirwinsky on the quantitative chemical and